EARLY SYNCHROTRONS IN BRITAIN, AND EARLY WORK FOR CERN

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Abstract
Early work on electron synchrotrons in the UK, including an account of the conversion of a small betatron in 1946 to become the world's first synchrotron, is described first. This is followed by a description of the design and construction of the 1 GeV synchrotron at the University of Birmingham which was started in the same year. Finally an account is given of the work of the international team during 1952–3, which formed the basis for the design of the CERN PS before the move to Geneva. It was during this year that John Adams showed the outstanding ability that later brought the project to such a successful conclusion.

1 EARLY PLANS IN BRITAIN: THE WORLD'S FIRST SYNCHROTRON

During the second world war Britain's nuclear physicists were deployed in research directed towards winning the war. Many were engaged in developments associated with radar, (or 'radiolocation' as it was then called), both at universities and at government laboratories, such as the radar establishments TRE and ADRDE at Malvern. Others contributed to the atomic bomb programme, both in the UK, and in the USA.

Towards the end of the war, when victory seemed assured, the nuclear physicists began looking towards the peacetime future. The construction of new particle accelerators to achieve ever higher energies was seen as one of the more important possibilities. Those working at Berkeley on the electromagnetic separator were familiar with the accelerators there, and following the independent invention (or discovery?) there of the principle of phase stability by Edwin McMillan in 1945, exciting possibilities were immediately apparent [4]. Indeed, even before this, Marcus Oliphant, while working on the electromagnetic separators at Oak Ridge, had put forward the idea of a ring magnet with frequency increasing with magnetic field to preserve synchronism, though he does not mention the essential feature of phase stability needed to make a very high energy machine a practical proposition [5]. His idea was to accelerate protons to an energy of order 1 GeV, where he guessed that 'quite new phenomena would be observed' [6].

During 1945 and 1946 there was active discussion between representatives from universities, industry, and at the newly created Department of Atomic Energy to decide what accelerators might be built. By the end of the year a programme had been established at Birmingham to build a proton synchrotron there, and electron synchrotrons of energy 300 MeV and 150 MeV were planned for Glasgow and Oxford Universities. In addition it was agreed that a few smaller machines of energy 30 MeV should be built by the English Electric Company to a design developed at the new Atomic Energy Research Establishment. These would be both prototypes for the larger machines, where the smaller aspect ratio of the vacuum chamber would impose tighter tolerances, and also be of interest for radiotherapy and for the study of high energy X-rays, and their application to the study of nuclear processes such as γ–n reactions and the photo-disintegration of nuclei.

The building of the Atomic Energy Research Establishment (AERE) on a disused airfield at Harwell had only begun in April 1946, and it was decided to start the synchrotron and linear accelerator programmes at Malvern, in a small self-contained area where huts had been erected during the war to house the top secret radar countermeasures group. The leader of the Malvern accelerator team was Donald Fry, who had been head of the microwave aerials group at TRE. He was responsible both for the synchrotron programme, and also for the design and construction of a disc-loaded linear accelerator, operating at 3 GHz powered by a magnetron developed for radar. At this time John Adams, also from TRE, moved to the new site at Harwell, where he played a major part in the design and construction of the new 175 MeV synchro-cyclotron which first operated in 1949. In overall charge of the synchrotron under Fry was John Gallop, an electrical engineer with industrial experience necessary for the large scale items such as the magnet and its power supply. Frank Goward and John Dain, both from TRE, were responsible for the overall machine physics and for the circuitry and controls respectively. Others had detailed responsibility for specific items, such as the vacuum system, RF system, and instrumentation.

By the time this team had become organized an American team at the General Electric Co at Schenectady in the USA was already well on the way to building what was to be the first synchrotron. Having already built several betatrons they were well acquainted with much of the technology required. At this time there was one betatron in the UK. This had been specially commissioned by A R Greatbach of the Woolwich Armament Research Laboratory during a visit in 1942 to Donald Kerst's Laboratory in the USA. He saw the possibility of using a small machine with sealed-off vacuum chamber for inspecting unexploded bombs that needed to be defused in situ. The betatron was designed by Kerst, and constructed in the University of Illinois workshops by Ernest Englund [7]. W H Koch, then a graduate student, assisted in the construction and tested the machine in its oil-filled container box in the University Electrical Engineering Laboratory towards the end of 1943. Early in the next year he took it to Woolwich and installed it there. By that time, however, conventional bombing had given way to attacks by the V1 'flying bombs' and V2 rockets, and the machine was not used for its original purpose.

At this point it is convenient to summarize the principle of the betatron with reference to Fig. 1. An alternating current at the supply frequency is passed through the coils; a pulse of electrons is injected from the gun at the instant that the magnetic field at the equilibrium orbit is such that the Lorentz force just balances the centrifugal force. The orbit radius then remains constant as the field rises and the particle accelerates, provided that the total magnetic flux through the orbit is twice what it would be if the field were uniform at all radii, (the Wideröe 2:1 condition). Betatron oscillations about the equilibrium orbit are stable provided that the field at the orbit falls off with radius, but less rapidly than 1/r. Near the peak of the magnet field the iron within the orbit is designed to saturate, so that the orbit radius contracts and the electrons spiral inwards to strike a target and produce X-rays.

Returning to the betatron at Woolwich, Goward realized that this could be converted to a synchrotron by increasing the magnet current, so that saturation occurred earlier in the cycle, and building a resonator around the vacuum chamber (or 'donut') in the form of a shorted quarter-wave line with a gap in the inner conductor, tuned to a frequency equal to the speed of light divided by the circumference of the orbit. At the betatron energy of 4 MeV the electron velocity was already within 1% of that of light. Then, just as the iron begins to saturate, the RF would be switched on, accelerating the particles by means of the electric field across the gap to a higher energy. This is illustrated in Fig. 2. Goward accordingly assembled an RF power supply from units available at TRE, and constructed a simple resonator. The form of the resonator is indicated in Fig. 3. If the resonator were made of metal tubes, as indicated in the figure, eddy currents induced by the changing magnetic field would distort the guide field and the beam would be lost. It was therefore constructed of wires, joined only at one point

by a planar ring parallel to the magnetic field. It was held together by dielectric spacers, and made in two halves which clipped together around the toroidal vacuum chamber. With this very simple equipment Goward, together with D E Barnes of Woolwich Arsenal demonstrated synchrotron acceleration for the first time in August 1946, two months before the General Electric machine operated in the USA. Electrons were accelerated from the betatron energy of 4 MeV to 8 MeV [9].

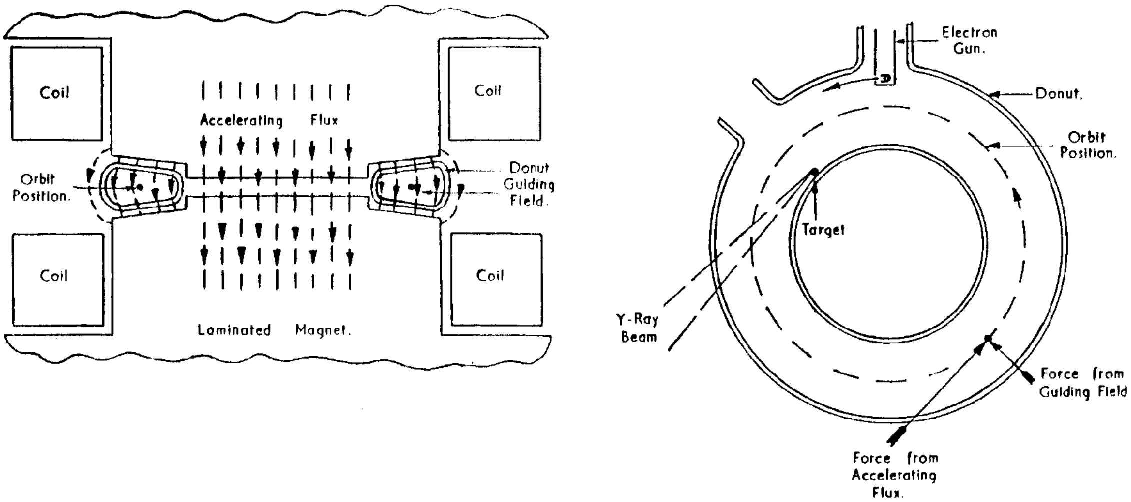


Fig. 1: Schematic diagram, showing essential components of a betatron, from Ref. [8].

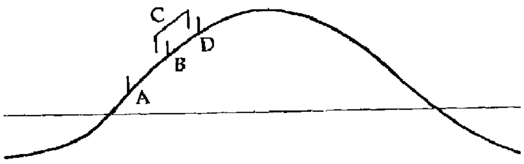


Fig. 2: Magnetic field variation during positive half-cycle, showing (A) injection pulse, (B) output pulse for betatron operation, (C) radio frequency envelope and (D) output pulse for operation as synchrotron.

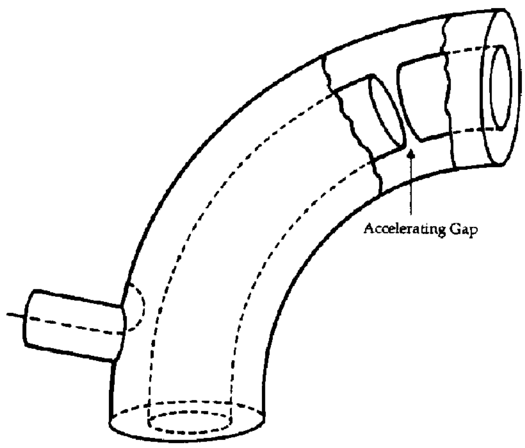


Fig. 3: Schematic drawing of a quarter-wave resonator. The actual resonator used was designed to fit round the vacuum chamber, and was constructed of wires to avoid eddy current loops. (Details in text.)

The machine was moved to Malvern, and by replacing the coils, adding air cooling and providing a DC bias field it was possible further to increase the energy to 14 MeV [10]. The X-ray intensity was greatly improved also by increasing the injection energy from 2 to 20 keV. A photograph of the modified machine from Ref. [10] is shown in Fig. 4. With these modifications it was used both for general experiments on synchrotron operation, and for experiments on medical applications. Extensive studies were made of the distribution of ionization in materials simulating human tissue, with various filters and collimators.

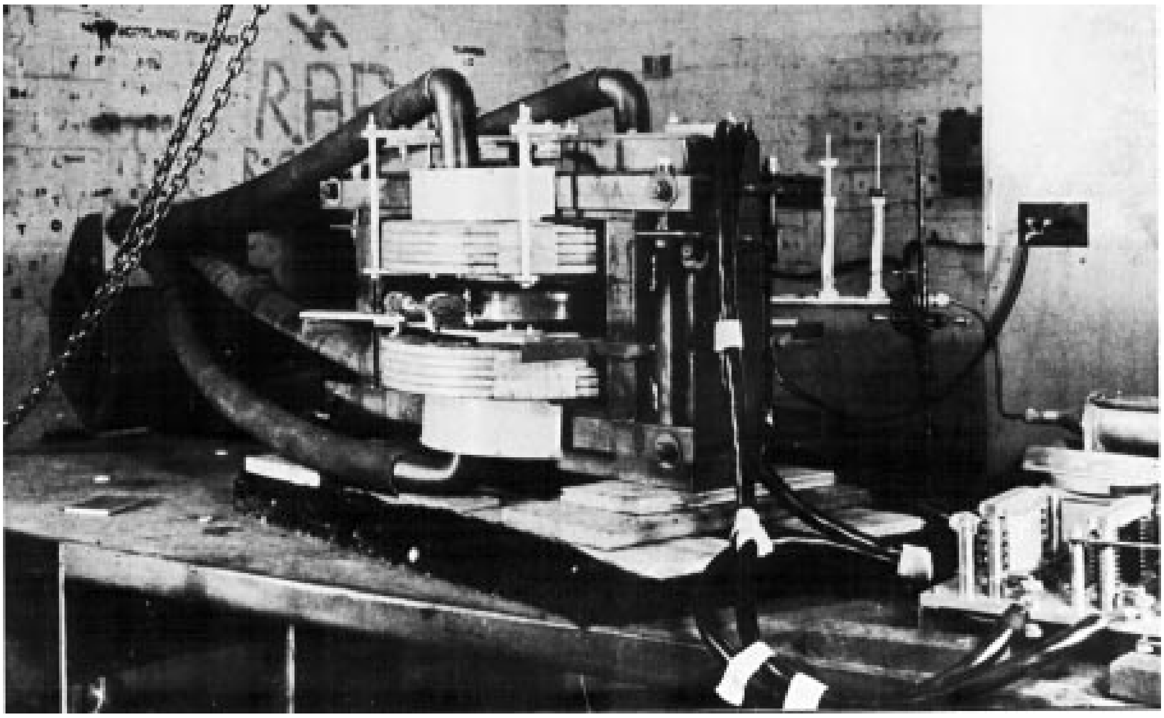


Fig. 4: The world's first synchrotron, installed at Malvern. The extra cooling system and RF feed to the resonator may be clearly seen.

2 DESIGN, CONSTRUCTION, AND OPERATION OF THE 30-MeV MACHINES

The practicability of synchrotron acceleration having been established by the end of 1946 by Goward and Barnes' experiment and the American General Electric machine, which first operated in October [11], what was now required to be done could clearly be seen. Construction of the first machine was well under way, and delivery of the magnet was expected during 1947. In January 1947 a fairly detailed specification of the parameters and work required had been prepared by Goward, Gallop and Dain. Some of the more important parameters of the first 30-MeV machine are tabulated below.

Energy at full excitation	30 MeV	Volts per coil	5 kV in series
X-ray output at 1 metre	10 Roentgens/min.	Current per coil	100 A rms
Injection energy	10 keV	Resonant capacity	30 μF
Orbit radius	10 cm	Quality factor (Q)	50
Field at maximum energy	1 T	Magnet weight	3 tons
Field index, n = -(r/B)(dB/dr)	0.7	Resonator frequency	477 MHz
Aperture of good field	6 cm square	Mean RF power	10 watts
Magnet coils	2 × 185 turns		

During the construction of the first 30-MeV machine there was activity analysing its expected performance, and that of the more critical larger machines. This was led by Goward, and a number of papers were published, particularly on pole face design, particle trapping at the betatron-synchrotron transition, the effects of magnetic field errors, and ideas for beam extraction [12]. This problem appeared particularly difficult, and a number of suggestions had been published in the USA, some applicable to betatrons, where beams had already been rather crudely extracted. Work was also done at Oxford in preparation for the machine there by Thomas Kaiser and James Tuck, who also performed

experiments on the 14 MeV converted betatron. Information from the American work, where papers on betatron operation had been published, and from the 70-MeV GE machine, which was working well, was also available. Eventually, after some constructional problems which delayed the delivery of the magnet until mid 1947, the first beam was obtained in October [13].

The design and operation of the 30-MeV machines, with both types of magnet, are described in two papers read before the Institution of Electrical Engineers in April 1950, and the numerous references to specialized detailed papers therein [14, 15]. Features of the larger machines, which would differ from the smaller ones, such as resonator and vacuum chamber design, and power supply, are included. Design information quoted below is from these papers unless referenced otherwise. Features of the larger machines, then at an early stage, are also described, since for several items, such as the power supply and vacuum chambers, different techniques are required. Photographs of machines with H and C magnets are shown in Figs. 5 and 6. The greater compactness of the second design is clearly seen, but it is evident also that the vacuum chamber was less accessible for experiments, and furthermore, it was necessary to remove one of the C units in order to replace it. Another feature of this design, seen in Fig. 6, is that azimuthal magnet inhomogeneities could be more readily corrected by ‘trim’ coils wound on the C’s. The magnet poles were designed using electrolytic tank measurements and numerical relaxation techniques to have a value of $n = -(r/B)\,(dB/dr)$ near to 0.7, to give a ratio of betatron oscillation frequency to rotation frequency $(1 - n)$ of order 0.5. Coils above and below the orbit carrying current proportional to the field were provided to enable the field gradient, and hence n, to be varied.

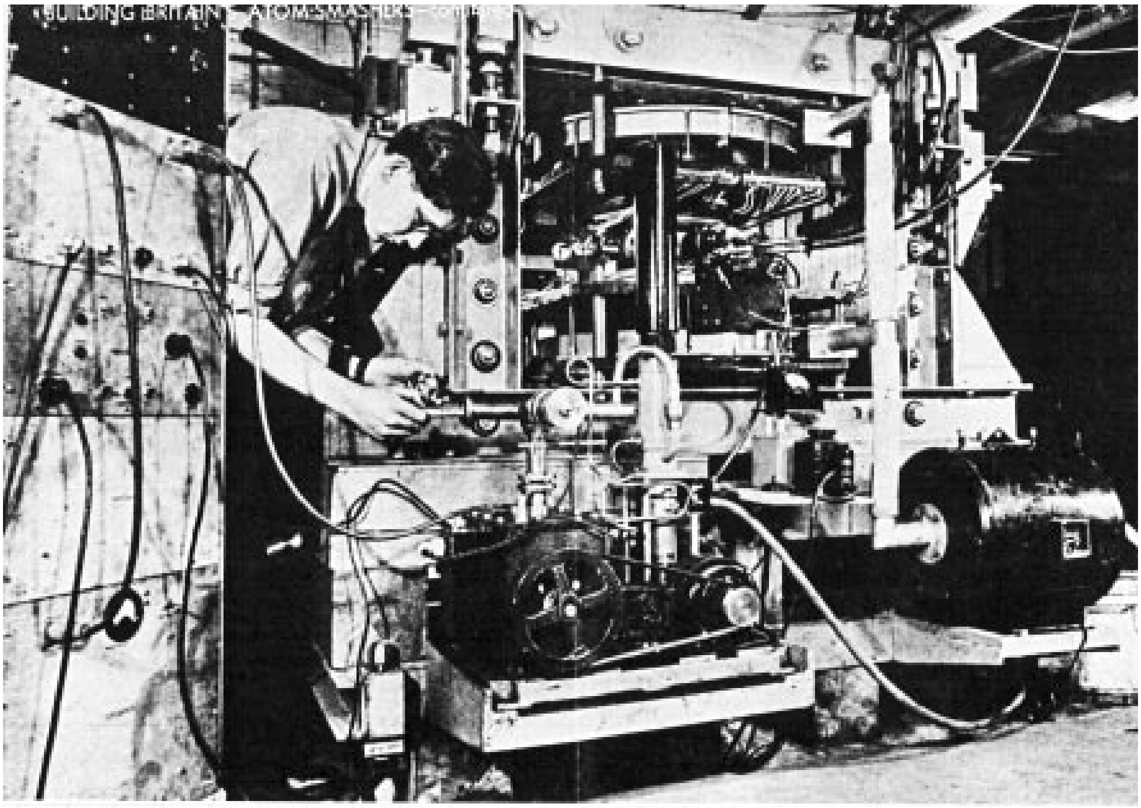


Fig. 5: First 30-MeV machine at Malvern, with H magnet [16].

The energizing circuit for both types of magnet was a series-driven resonant circuit at the supply frequency controlled by a large manually adjusted variable ratio auto-transformer (‘Variac’). ‘Metrosil’ was included for emergency voltage limitation, and trimming capacitors plus variable inductance were

included for fine tuning. This was very necessary at the time, since the mains frequency was by no means stable; after 5 p.m., when the industrial load was shed, the frequency increased; it was allowed to rise so that the total number of cycles in a 24 hour period was the same as if there had been no variation from 50 Hz.

The accelerating field was provided by a quarter-wave line resonator, made of silver plated on ‘Faradex’. This is a ceramic with high dielectric constant, so that the resonator length was only 2 cm, enabling it to be easily inserted through the side arm. The silver coating was 20 microns thick, with a circumferential strip etched away to provide the accelerating gap. The coating was sufficiently thin that eddy currents produced negligible perturbation of the guide field. The Q-factor was 500 at the operating frequency of 477 MHz. The resonator was water cooled, and fed with a peak power of 60 watts, which provided 100 volts across the gap. The voltage gain per turn required for acceleration was about 20.

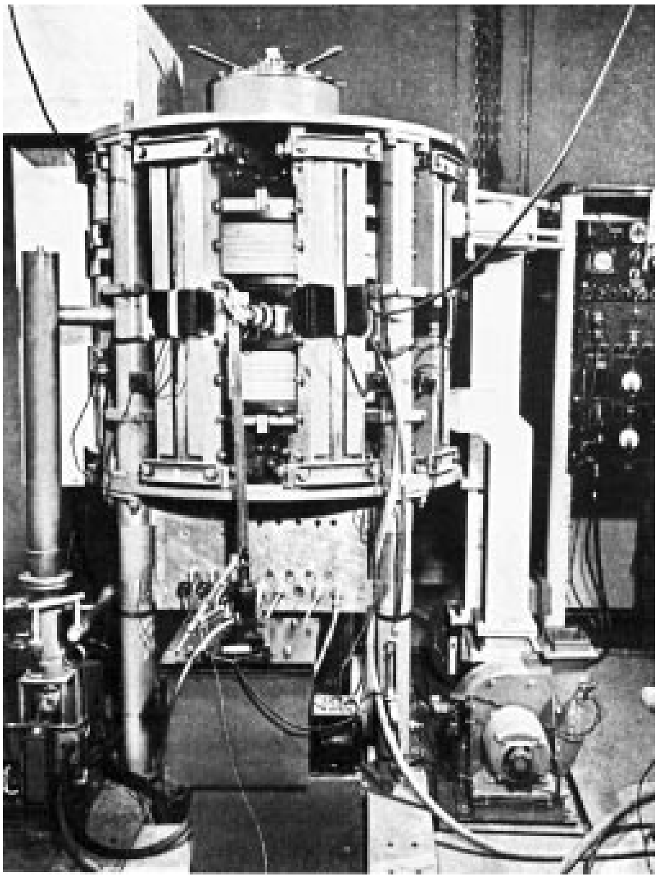


Fig. 6: Second machine at Malvern, with C magnet. The greater compactness of this design, but reduced accessibility to the vacuum chamber is evident

The injector gun was based on Kerst’s original design. The cathode was a helix of 0.25 mm tungsten wire mounted within a semi-cylindrical ‘Wehnelt’ electrode, all of which was pulsed negatively, allowing electrons to pass through the vertical 1.8 mm gap in the surrounding earthed molybdenum shield. The gun could withstand up to 40 kV.

The original vacuum chamber was made of two flat circular Pyrex plates with circular holes, joined by black vacuum wax to two cylinders, the outer of which had side arms to accommodate gun, ionization vacuum gauge, resonator and vacuum outlet. The interior was roughened by sandblasting, and an earthed film of Nichrome evaporated on to it. Lack of, or damage to, this film allows charge to accumulate which inhibits injection and capture into stable orbits. This type of chamber was soon replaced by a more satisfactory ‘blown’ design, ingeniously constructed by GEC from large borosilicate glass cathode-ray tubes. The centre of the face, and the neck of the tube were heated to softening point and pushed together to form a ‘donut’ shaped tube. The side-arms, which were larger than in the original design, were sealed on mid-way through this operation. Three of them were fitted with ground glass flanges for water-cooled greased vacuum joints. Platinum was fired on the inside to provide the conducting coating.

Pumping was from 2-inch Metropolitan-Vickers diffusion pumps using Apiezon B oil, with cone joints sealed by ‘J-oil’. The pumping line was attached by a waxed joint and sylphon bellows to the unflanged side arm. The pumping speed of 10 litres/sec at the vacuum chamber produced an operating pressure in the range of 2 to 10×10^{-6} torr. The pressure was measured by an ionization gauge

improvised from a First World War R1 army triode, and the backing pressure by a Pirani gauge initially improvised from an electric light bulb. Phosphorus pentoxide traps were used to remove water vapour, and a feature that would horrify modern safety officers was the use of liquid oxygen in the cold traps, in close proximity to the hot oil. Liquid nitrogen was not available commercially at the time.

The control circuitry used many of the features that had been developed for radar applications during the war. An additional feature, however, was the use of high permeability saturable peaking strips which could be set to respond at a pre-determined magnet current by varying the bias current. Finally, an integrator was used to provide a timebase proportional to the magnetic field, on which were displayed zero field, injection pulse, RF pulse and X-ray output. The forward sweep was during the rising field (0–90° phase) and the backward one, from 90°–180°, was displayed below it. The negative half-cycle was not shown. This display is exhibited in Fig. 7 (from Ref. [14]) together with a photograph from Ref. [13]. The X-ray output was indicated by a Geiger counter, a quantitative measurement of the average output being provided by an ionization chamber. Two pulses may be seen; the later one is at the time expected, the origin of the earlier one will be explained later. Experiments on this machine are described in Section 6, after a diversion on other activities during 1947–8.

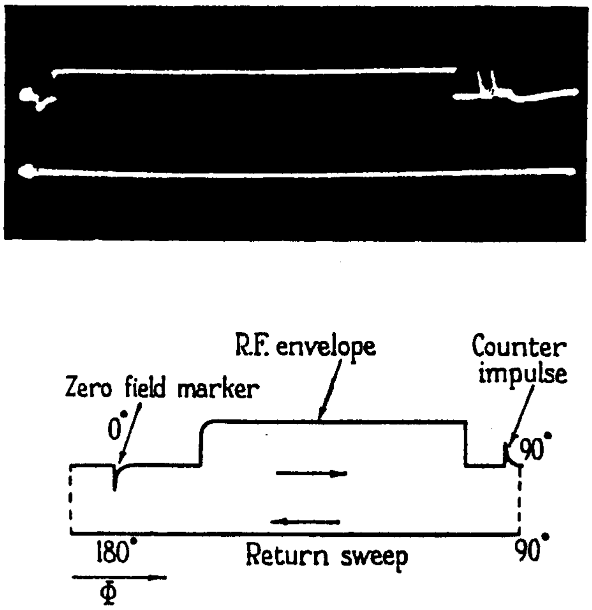


Fig. 7: Display of injection pulse, RF, envelope and Geiger counter output [13, 14].

An occasional visitor to Malvern during the early days was Olle Wernholm, who was constructing a machine of similar energy at the Royal Institute of Technology in Stockholm. This was under the overall direction of Hannes Alfvén, who was active in early discussions on the formation of CERN. The machine was initiated in 1948 (after earlier experiments with small betatrons), the energy of 35 MeV being determined by the size of magnet that could conveniently be handled in the laboratory. It operated at full energy in 1951, and apart from the Russian machines it appears to be the only other synchrotron in Europe that became operational during the period covered by the present report. It is fully described in Ref. [17]. It differed in several respects from the Malvern machines; the good field aperture was less (about 4×3 cm), the orbit radius was 20 cm, with consequent lower peak field and accelerating frequency.

The magnet was compact, and weighed 1.3 tons, with 20 C-sections. The betatron flux was provided by a series of bars just within the vacuum chamber position, in contrast to the solid central core in the Malvern machines. The resonator was similar in design to that used by the American group at GE [11]. Injection was at 18 kV, using a gun similar to that used by Kerst and on the Malvern machines.

The X-ray output was somewhat less than that at Malvern, but the design and configuration more economical, and more similar to that used for building larger machines elsewhere.

3 A FAILED EXPERIMENT, LINKS WITH FUSION, AND AN IMPRACTICAL SUGGESTION

At this point some ‘dead ends’, which commonly occur, but are rarely recorded, will be described.

First, it should be mentioned that close links were kept with the Birmingham synchrotron in the early days. Discussions on theory and common problems were often held. An essential difference between the proton machine there and the electron machines was that the former required that the frequency be varied over a large range during acceleration. This problem seemed especially difficult because the change was required to be most rapid at lower energies where the frequency was low, whereas any mechanical tuning device required relatively large movement at the low-frequency end.

The idea of making an electron model with frequency modulation rather than betatron acceleration, was put forward by Goward, and early in 1947 John Lawson was recruited from TRE and given the problem of making the model. This was to have the same pole shape and dimensions as the 30-MeV machines, but with a slow rise time of one second and maximum energy of 3 MeV. This would require a small magnet yoke, and radial slots in solid iron would suffice to prevent eddy currents. The gun and vacuum system would be the same as for the 30-MeV machines, and because of the low peak field and slow rate of rise the power supply would be small.

Unfortunately this project was embarked upon in the wrong way. Instead of an overview of the whole scheme being taken to see where the greatest problems would arise, it was tackled piecemeal. The magnet, which would take the longest time for manufacture was designed and ordered, and experiments were undertaken to make an oscillator covering the required frequency range. A butterfly oscillator with grounded-grid triodes was completed which covered the range of 100–500 MHz, and a matched accelerating electrode designed on the (unjustified) assumption that a very small accelerating voltage would be adequate to provide the 12 mV per turn needed for the very slow rate of acceleration. After this stage unconsidered problems began to appear, such as the design of a mechanism to drive the butterfly shaft with the right frequency-time characteristic, and the need for exceptionally good vacuum to avoid gas scattering. These were found to be so severe that the project was cancelled. This was just at the time that the C magnet and second 30-MeV machine was commissioned, and Lawson was given charge of the original H-magnet machine and asked, among other things, to extract the beam.

During work on the frequency-modulated machine an interesting proposal was made by Sir George Thomson of Imperial College, who was working on early ideas for controlled thermonuclear reactions in a toroidal tube containing hot plasma isolated from the walls by magnetic fields [18]. Following suggestions of Rudolph Peierls at Birmingham he decided to investigate the possibility of confinement in the field of a very large current circulating in a torus. This would be continuously injected from a gun, and space-charge forces which normally limit the current would be neutralized by ionizing residual gas in the torus. Although the details were not yet clearly thought out, the problem of gas scattering in air and hydrogen was studied experimentally in the 30-MeV machine, and shown to disperse the beam before appreciable ionization could occur. The result of these experiments, but not the reason for doing them, was published [19]. Most experiments were conducted with air as the background gas, but hydrogen was also tried and found to be roughly equivalent to air at one-tenth the

pressure. The scattering problem would, of course, be reduced if the acceleration were more rapid, and Thomson instigated a programme to build an ironless betatron with very rapid rate of field rise at Imperial College. Some details of the work were published, but again not its object [20, 21]. He also suggested that the betatron might capture a greater current if a toroidal winding carrying constant current were wound round the vacuum chamber. This appears to be the first suggestion of this scheme, now known as the 'modified betatron', which has been much studied recently as a potential high-current device. The problems of injection and extraction have proved to be intractable, however, and no useful device has been built. The experiment was done on the 14 MeV converted betatron, but the current decreased in the presence of the azimuthal field. The theory was worked out for the first time by Walkinshaw, who showed that the field produces coupling between vertical and horizontal betatron oscillations, giving rise to normal modes whose projections on a plane through the vertical axis are elliptical rather than horizontal and vertical straight lines [22]. For the parameters of the experiment this would reduce the injected current.

Another early idea for a proton synchrotron avoiding the use of a continuously time-varying radiofrequency system was the 'harmonic synchrotron', proposed by Kaiser and Tuck at Oxford, and independently by R B Robertson-Shersby-Harvie at Malvern [23, 24]. In this scheme acceleration is by a resonator operating at a high harmonic of the orbital rotation frequency, $\omega_g = m\omega$. As the particle velocity increases the orbit radius increases also; after a suitable time the accelerating field is switched off so that the orbit radius then contracts to its original value. This is arranged to occur when $\omega_g = (m - 1)\ \omega$ after which the process is repeated, so that $\omega_g = (m - 2)\ \omega$ and so on. If m is always large the radial excursion can be kept small. More than one gap can be used provided that the relative phases at which the gaps are fed are adjusted to give a rotating wave with the required phase velocity. If this is done, however, some particles are inevitably lost at each transition. The scheme is obviously complicated, and no machine of this type appears to have been designed.

4 EXPERIMENTS IN 'MACHINE PHYSICS'

As soon as machines became operational there was intense activity in measuring their characteristics, varying the parameters to see how critical they were, and comparing with expectations from the fairly detailed theory of betatrons and synchrotrons that had already been published in the USA [25].

By the time the 30-MeV machine first operated much had already been done on the American 70-MeV machine, and furthermore, several problems such as the effect of field errors and the important and difficult question of injection efficiency had already been studied by Kerst and others in the USA on betatrons. A brief history of American work and list of references is given in the book by Livingston and Blewett [26]. Experiments on the 14-MeV machine are described in Ref. [14] by the authors and by Kaiser and Tuck from Oxford [27]. Work on the later 30-MeV machine is described in Refs. [14], [15].

The precise mechanism of injection is unclear. It is evidently some collective effect, since with the parameters of these machines ballistic theory predicts that the rate of orbit contraction is so slow that the injected electrons would hit the back of the gun after a few turns. Indeed, if the injected current was progressively reduced by lowering the cathode temperature it was found that a cut-off existed below which no electrons were injected. The injection problem was much studied, particularly (rather later) by Soviet workers. Interested readers should consult the 100 page article by Gonella, which contains over 300 references (and also a list of 43 electron betatrons and synchrotrons) [28]. Following experiments on a betatron in the USA by G D Adams [29] a further experiment on the 30-MeV

machine, in which a rapidly pulsed 'orbit contraction coil' produced a rapidly increasing field in the magnet at the time of injection, showed no cut-off, but produced no increase of current at full gun emission. A similar type of device on the later Oxford machine produced a substantial increase in output [30]. It was also found (on the 30-MeV machine) that the effective vertical aperture, found by inserting a moveable horizontal wire, was greater when the orbit contraction coil was used. Numerous other experiments, described in Refs. [14], [15] unless otherwise indicated, were performed. The timing and length of the injected pulse, and the position of the gun were systematically varied; it was found, for example, that injection from inside the equilibrium orbit was equally efficient. The n value at injection was also varied by a pulsed coil attached to the poles above and below the orbit radius. Azimuthal harmonic errors in the field were deliberately introduced at injection, again by suitable windings attached to the pole faces, and the aperture constricted in various ways to find out how important these factors were. Comparison with theory was made where possible. The dependence on resonator frequency and power was also measured. A series of experiments on the effect of pulsing the RF power off for short periods was performed on the 14-MeV machine and compared with theory [27].

A suggestion as to how the puzzling double pulse illustrated in Fig. 7 might arise was made by Lawson [31]. This arose by analogy from the observation that in the evening the magnet excitation would suddenly drop to a very low value. As the industrial load was shed from the supply network, the frequency, which was just below 50 Hz during the day, began to rise. Since the magnet represented a non-linear inductance which decreased with current amplitude, the resonance curve for the magnet circuit was of the form shown in Fig. 8; two states of excitation were possible over the frequency range between the dotted lines. As the frequency gradually increased the excitation followed the path ABCD. Between B and C there was a sudden drop in amplitude. (For a decreasing frequency the path DEFA would be followed, showing a hysteresis effect.) During operation resonance was restored by removing the excitation, switching out a small fraction of the condenser bank and restoring the excitation, so that the resonance curve was shifted as shown.

Returning to the double pulse, the n value of the magnet is roughly 0.75, giving $Q = \sqrt{(1-n)} \approx 0.5$ so that about half a cycle of betatron oscillation occurs per revolution. If now there is a perturbation at some azimuth arising from an error in the n-value, resonant build-up occurs. If, in addition, the oscillation is non-linear, as in Fig. 8, and exact resonance occurs for a finite amplitude of oscillation, there will be two stable orbits: the normal one and another which closes after two turns, as shown in Fig. 9. (A 'phase-plot' is shown in Fig. 10. Such diagrams were of course unknown to us at the time of this experiment.) If at injection some particles are captured into each orbit, and further, the orientation of the target is as shown, then the particles in the orbit that closes after two turns will hit the target before those in the normal orbit, giving the double pulse shown in Fig. 7.

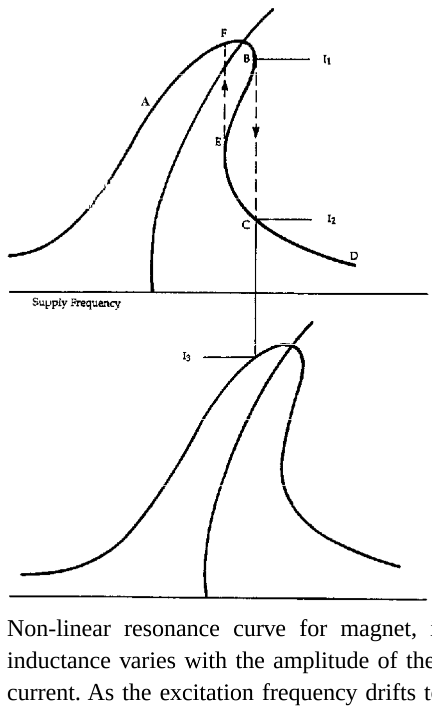


Fig. 8: Non-linear resonance curve for magnet, in which inductance varies with the amplitude of the exciting current. As the excitation frequency drifts to a value f_1, the current amplitude drops suddenly from I_1 to I_2. Removing condensers from the resonant circuit shifts the resonance curve to higher frequencies, with current of I_3.

This hypothesis was tested by a simple experiment. By walking round the machine carrying a piece of iron (a small transformer) it was possible to vary the position and amplitude azimuthal perturbation and the relative amplitudes of the two pulses. Indeed, by standing in suitable positions it was possible to make either disappear completely. (Such an experiment would have taken much longer with modern regulations on radiation protection!)

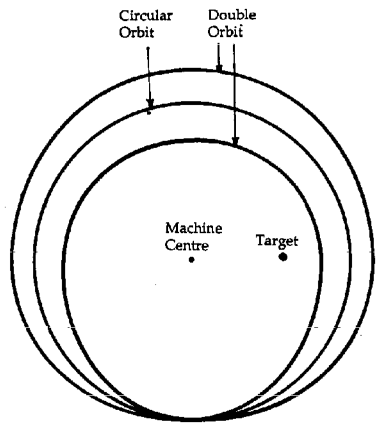


Fig. 9: Two stable orbits in synchrotron with n ~ 0.75, non-linear restoring force, and harmonic perturbation. Particles oscillating about the 'double orbit' hit the target first as the orbit contracts after the RF has been switched off.

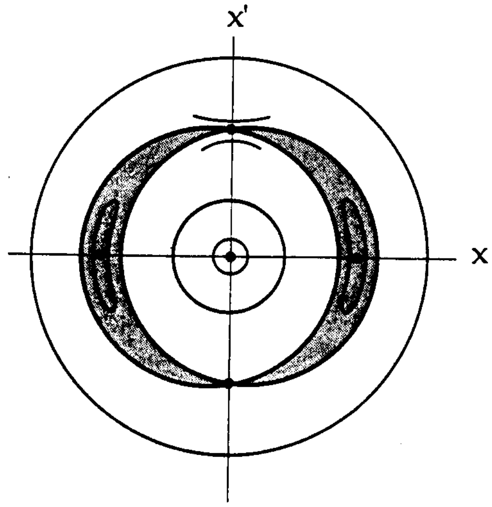


Fig. 10: Schematic sketch of phase-space diagram for machine with double orbit. Coordinates x and x′ are plotted at the same azimuth on successive revolutions. The shaded area represents the double-orbit regime, with successive points lying on curves in the two parts, which enclose a pair of stable fixed points. There are unstable fixed points where the separatrix curves cross, and a stable fixed point at the centre.

5 BEAM EXTRACTION

Although several schemes for beam extraction were proposed and analysed, this was found to be rather difficult and met with only limited success. Indeed, extracted beams were not obtained on the Glasgow and Oxford machines, and the scheme developed at Malvern was inefficient and never used for experiments. In this scheme the orbit radius was first expanded by reducing the field by means of pulsed concentric coils in the magnet gap. At a sufficiently large radius, where the rate of radial decrease of field was sufficient to make $n > 1$ the orbit became unstable so that the electrons spiralled rapidly outwards, and entered a pulsed magnetic shunt consisting of four parallel conductors arranged in a square of side 2 mm. The pulsed field cancelled the magnet field locally, producing an approximately tangential line with zero field with stable radial focusing. This was sufficient to deflect the beam out of the magnet field.

Details of the design are given in Ref. [32], and operation at 20 MeV is described in Ref. [33]. The beam quality was rather poor, the extraction efficiency being estimated as being between 15% and 50%. Further development (including a modulator with longer life valves) was needed to make the beam usable for experiments, but owing to the closure of the programme at the end of 1950 this was not carried out. The difficulty of extracting the beam represented a major disadvantage with respect to linear accelerators in a comparable energy range.

6 EXPERIMENTAL PROGRAMMES ON THE ELECTRON SYNCHROTRONS

A detailed description of the various experiments carried out at Malvern and on the two medical 30-MeV machines is outside the scope of this report, nevertheless a few comments (without references) will be made. The 14-MeV machine was used exclusively for the medical studies on the distribution of ionization in targets of various materials and geometrical configuration produced by the X-radiation from an internal target, yielding empirical information needed for cancer treatment. Similar work was done on the 30-MeV machine operated by the Medical Research Council in Cambridge, but abandoned after it was found to be unlikely to offer real advantages over conventional X-ray therapy.

The principal series of physics experiments on the 30-MeV machine at Malvern was on photo-disintegration of the light elements, particularly the $\gamma + C \rightarrow 3\alpha$ reaction and photo-fission of uranium, both using the nuclear emulsion technique that had been developed at Bristol for cosmic-ray studies. Thresholds for γ–n reactions were measured for a number of elements, but attempts to determine the shape of the 'giant resonance' curve were not successful. It is possible to measure neutron yield as a function of peak X-ray energy, but finding the shape of the resonance curve involves the solution of an integral equation, and this requires very accurate data, especially of the shape of the distribution at the top end of the bremsstrahlung spectrum. Despite several proposals, no accurate measurements of the spectral distribution could be made, so theoretical values were used. Measurements were made of the angular distribution of the X-radiation of the target, and fair agreement was found with theory, which involves a convolution of the angular distribution from multiple scattering at various levels in the target with the angular distribution of radiation associated with a single radiative collision.

An ionization chamber with thick walls and disc-shaped air volume was constructed, and the response to a theoretical bremsstrahlung spectrum as a function of energy up to 30 MeV calculated. Using also the knowledge of the angular distribution of radiation it was possible in principle to measure the current striking the target in the synchrotron.

The synchrotron development programme at Malvern was terminated at the end of 1950. By this time it was realized that linear accelerators provided a more intense, reliable, and accessible beam for physics experiments and medical work for energies up to 30 MeV. Furthermore, the basic work and expertise required for the Glasgow and Oxford machines had been completed. A third reason was that the Korean war had started, and priorities returned to defence. A number of staff, including the author, were abruptly moved to defence-related work.

The Oxford and Glasgow machines duly came into operation in 1952 and 1954, and ran for a number of years. The former was a disappointment in that it just failed to reach the π-meson threshold; this was partly due to distortion in manufacture that limited the energy, but more to the fact that at the time the project was initiated π and μ mesons had not been distinguished and the π-meson mass was therefore higher than expected.

The Glasgow machine worked well, and though operation was rather later than that of similar machines in the USA, useful work was done. More details of these machines are given in Ref. [1].

The Swedish machine was moved from Stockholm to Malmö, where it was used first by radiobiologists, but later by physicists to study photonuclear reactions. It is now in a museum at Malmö.

7 THE DESIGN AND CONSTRUCTION OF THE BIRMINGHAM PROTON SYNCHROTRON

In a memoir written in 1967 for the Physics Department of the University of Birmingham Oliphant has described how the idea for this machine came to him at Oak Ridge during 1944 while he was on night shift tending the electromagnetic separators [5]. Oliphant had worked with Rutherford at the Cavendish in the mid-thirties and had built a 200 kV accelerator for their classic experiments on the D-D reaction. Later, as Poynting Professor of Physics at Birmingham, he had initiated the construction of the Nuffield cyclotron shortly before the war. He returned to Birmingham in 1945 intent on finishing the cyclotron and building the synchrotron. An early document ‘The Acceleration of Particles to Very High Energies’ (post-dated September 1943, though it seems that this should be 1944) survives [6]. This clearly describes a ring-magnet accelerator, in which the frequency is varied with the magnetic field to keep the orbit radius constant. Radio-frequency electrode systems and the practical problem of frequency variation are considered, and suggestions made on methods for beam injection and extraction. No comment is made on focusing, however, either in the magnetic field or radio frequency, (phase stability). Nevertheless, in his memoir Oliphant gives the impression that he understood both the conditions for radial and phase stability at this time [5]. Iron and ironless magnets were discussed, and acceleration of both electrons and ions considered. For protons, a specific energy of 1000 MeV was quoted, with the possibility of injection from the Nuffield cyclotron at an energy of 45 MeV. At the time a machine of such high energy was a very bold proposal, illustrating Oliphant’s visionary approach, and urge to explore entirely new territory. He was convinced that ‘new and important phenomena would be discovered’[6].

This document, (or a similar one) together with a further one detailing some changes [34], were presumably presented as support for the application resulting in the award in 1946 of £140,000 by the Department of Scientific and Industrial Research (DSIR) for the construction of the machine at Birmingham. To save time and money no new building was planned, but the machine was to be put in the large room originally intended as an experimental area for the Nuffield cyclotron. This cramped location was later found to be very restrictive; space for the extracted beam and experiments turned out to be very limited. It was, of course, hoped that this would be the first machine to operate in this energy range, so that high beam intensity and precision experiments would not yet be required. The emphasis was to be on speed and ingenious improvization with as little detailed planning as possible. This approach was well suited to Oliphant’s work with Rutherford, but its shortcomings in a project of this size where large-scale engineering was a major component soon became apparent.

An initial grant from the Nuffield Foundation, before the DSIR money was available, enabled a team to be assembled and exploratory work to begin. Practical aspects of the various components were considered in more detail, and the synchrotron theory already published in the USA [25] was extended and adapted to the specific problems of a machine in which the frequency varied over a wide range during acceleration. Trade-offs and tolerances were considered, and a somewhat over-elaborate phase equation derived. This initial work is described in two papers published in 1947 [35, 36].

After rejecting a resonant ironless magnet on account of the very high cost of the capacitors that would be required, a steel ring magnet of orbit radius 450 cm weighing about 800 tons was chosen; this was to be constructed of $\frac{1}{2}$ inch radial plates of low-carbon steel thin enough to avoid eddy current field distortion of the magnetic field, for a rise time from zero to 1.5 Tesla in about one second. A coil winding with 22 turns carrying 11 kA driven at 1.1 kV by twin-coupled motor generator sets was envisaged. The magnet current would be driven back to zero by reversing the generator field current, the speed being kept sensibly constant by means of a 36 ton flywheel. There would be one pulse every

10 seconds. Injection of protons was planned at 0.3 MeV implying a frequency change during acceleration from about 0.27 to 9.3 MHz, a factor of 34. Single-turn extraction using electrostatic deflection was envisaged, though never built, as explained later. An eight-section ceramic vacuum chamber was planned, though a 60-section system was ultimately used. No mention was made in these early papers of what was to be one of the more challenging problems, the provision of a radio-frequency system with the required 34:1 frequency range.

Formal progress meetings had already begun in 1946, after John Gooden had been appointed project leader. He was one of the many Australians besides Oliphant who was to make an important contribution to the project. These meetings are meticulously documented in the minute book by the secretary D F Bracher, whose early reminiscences are documented in the Proceedings of the 1993 Anniversary Meeting [37]. The project moved ahead, but the sheer amount of effort that would be required was beginning to be apparent. The rather small-scale engineering and technical support meant that many of the physicists participated in detailed design decisions, and spent time supervising and taking part in actual construction and installation work. This was particularly so in the early days of the magnet installation. Oliphant always believed that conventional engineers were too conservative, and was ready to flout conventional practice to save time. This gave rise to some tensions, but the local workshop staff were very flexible and contributed enthusiastically without undue formality. Most of the team were swept along by Oliphant and Gooden’s infectious enthusiasm, and despite occasional opinionated disagreements, worked well together. Oliphant had originally hoped for completion in 1950, but as time passed it was soon appreciated that this was unrealistic.

During 1947–8 the synchrotron passed from design to construction, and the main magnet steel work was erected. During the following year the copper coils were wound, and tested with the newly installed generator. Meanwhile work was proceeding on other aspects of the machine, particularly the very challenging radiofrequency system, for which L U (Len) Hibbard, assisted by David Caro later took responsibility. John Symonds contributed in various ways, applying the theory of Ref. [36] to injection studies, calculating gas scattering and vacuum requirements, and building the pulsed ion source. This was fitted to the 500 kV Philips HT set which had originally been used for nuclear physics experiments. Len Riddiford arrived to take charge of the vacuum system; after heated arguments between him and Hibbard on the one side and Oliphant on the other, ceramic was chosen rather than corrugated stainless steel. Several test sections were ordered in relatively inexpensive chemical stoneware; this was found to be much too porous, and electrical porcelain was chosen for what was, at the time, a very large vacuum system for such low pressures.

The year 1950 was a disheartening one. First came the untimely illness and death of John Gooden, to be followed shortly by the departure of Oliphant. He felt that his loyalty was primarily to Australia, and left for Canberra in July to set up the physics department at the new Australian National University and there embark upon his ill-fated 10 GeV machine. The background to these events is presented in the biography of Oliphant by Cockburn and Ellyard, where the personal and organizational factors involved are discussed in some detail [38]. Further comment may be found in the history of the Birmingham Physics Department by Moon and Ibbs [39].

In this year Hibbard wrote a paper giving an overall description of the machine, including many diagrams and a table of the main parameters [40]. This is the most complete overall description that exists, though of course it is not up to date in some details, particularly of the radiofrequency, vacuum chamber, and extraction system. In it parameters foreseen at the time (but not all achieved) are tabulated. Sketches of the magnet from Ref. [40] are shown in Fig. 11. Details of the magnet cycle, and the power supply and triggering circuits are also given in Ref. [40], essential features being an almost

linear rise of magnetic field from zero to a maximum of 1.5 Tesla in 1 second, triggered every 10 seconds by a signal from the variable frequency RF generator at the appropriate time to an accuracy of 1 msec.

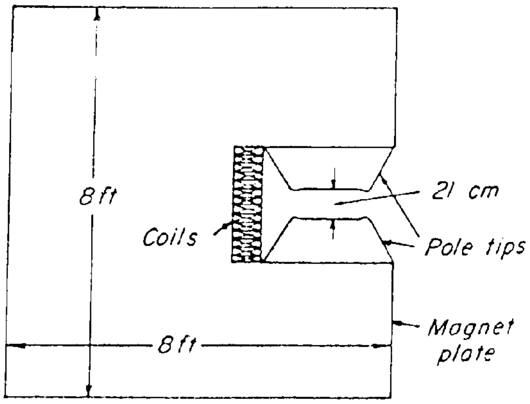


Fig. 11a: Cross section of Birmingham synchrotron magnet plate [40].

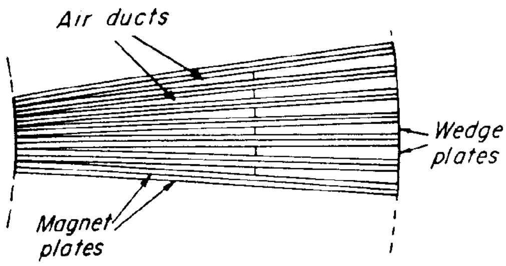


Fig. 11b: Plan view of a section of the magnet showing pairs of flat 1/2" magnet plates angle-spaced by short wedge plates [40].

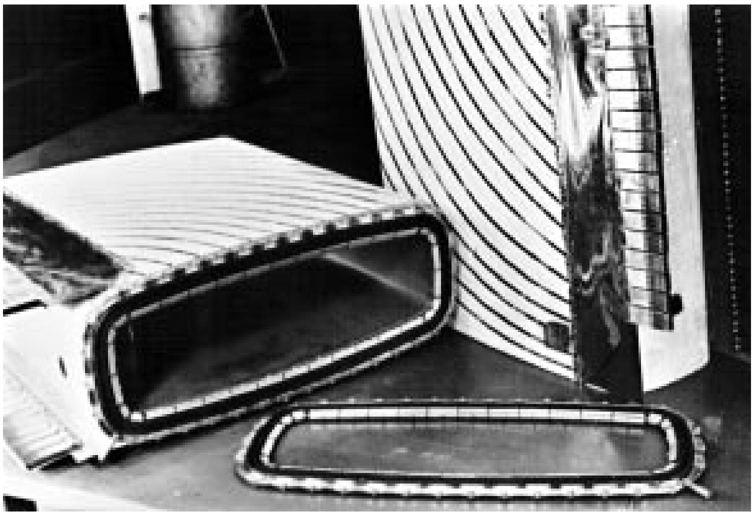


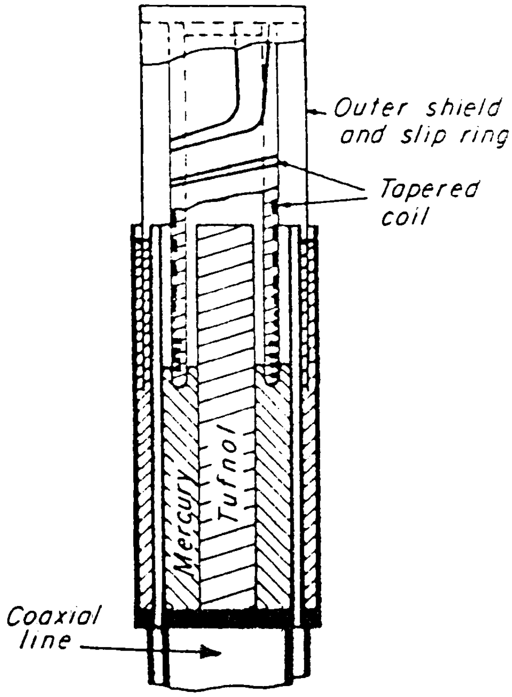
Fig. 12: Porcelain vacuum sections at the centre of the Cee, showing the laminated silver coating leading out to the spring contacts of the sliding joint. Brass gasket plates with moulded rubber rings used for vacuum sealing can be seen. Electrical connection between the porcelain sections is effected by the two sets of spring contacts carried by each gasket [43].

The 60 sections of the porcelain vacuum chamber were coated internally to prevent charge accumulation, and joined together with double rubber gaskets. The accelerating electrode was in the form of a centre-fed 'Cee' extending over an angle of 96°, in which circumferential strips of copper were sprayed on the outside of the vacuum chamber, (Fig. 12). This, together with thin copper foil glued to plastic and mounted on the magnet pole face, produced a 5 ohm transmission line, and was fed through a wide-band transformer with a core of very thin wound mu-metal [41].

With Oliphant's departure at the beginning of July, responsibility for completing the synchrotron fell on Moon, soon to be appointed Poynting Professor. Neither particle accelerators nor high energy physics were close to his current interests, and although he was not happy to be 'landed' with the project, he tackled it conscientiously and with vigour. It was a difficult year, and some of the problems were proving less tractable than anticipated.

Furthermore, lack of technical support was causing some of the installation work to move more slowly than planned. Indeed, the original hope for completion by 1950 could clearly not now be realized.

One of the more challenging problems was provision and synchronization with the magnet field of the variable accelerating frequency. Nothing quite like it had been tackled before; the initial low energy stage when the frequency is low and changing rapidly is particularly difficult. Tolerances are tight, and the resonant tuning of the capacitative Cee requires a rapid and large change of inductance by a factor of 1000 in a coil in parallel with the Cee. This was accomplished by plunging a very non-uniformly wound cylindrical coil into a pot of mercury at high speed, where splashing and scum formation presented problems that needed much ingenuity to solve [42]. This is shown in Fig. 13. In addition, the frequency had to be generated accurately at low power and then amplified. The tolerance at low frequency was $\pm$ 0.1%, and the variation had to follow the rising magnetic field. This implied that the field variation with time had to be known accurately, and the initial synchronization needed to be good. The required frequency was generated by beating together two oscillators, one with fixed frequency, and one with frequency controlled by a variable condenser, one plate of which was a very carefully machined rotating disc.



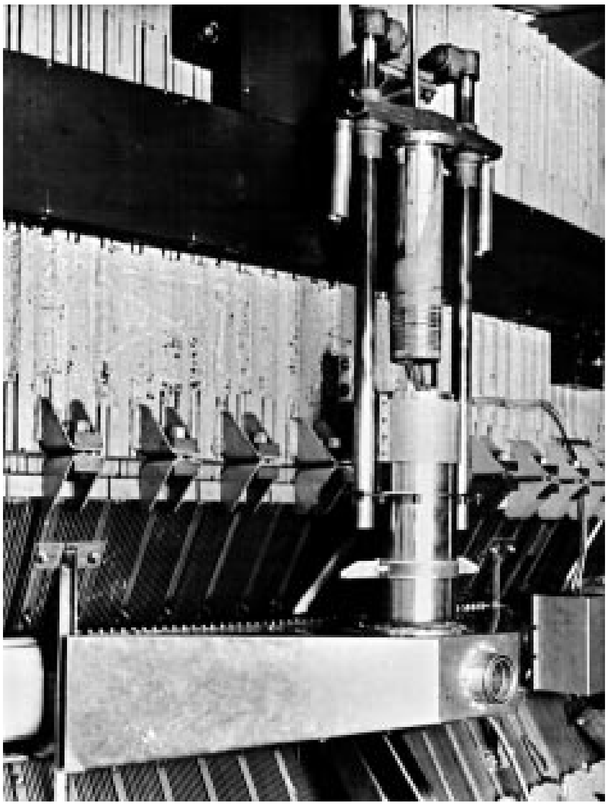
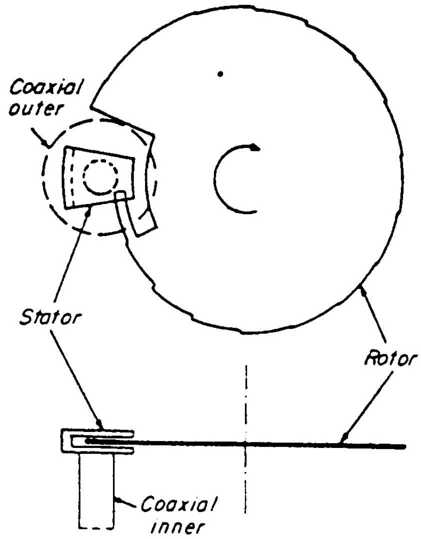


Fig. 13: Variable inductance for tuning the accelerating electrode. A tapered coil is plunged into mercury and the inductance varied by a ratio of 1000 to 1. (From Refs. [40], [43].)

In order to cope with slight variations between magnet cycles the disc was driven by a servo motor. Information on the position of the disc was obtained from 120 strip mirrors placed with extreme accuracy around the circumference of the disc. The time between successive pulses of light reflected from these mirrors gave a measure of the angular velocity, and the servo ensured that the angular velocity corresponded to the correct magnet field. This was determined by integration of the e.m.f. across a coil in the magnet gap. Very tight tolerances, both mechanical and electrical, were required on

all aspects of this system, which in a sense was the ‘heart’ of the machine. Full details of this very elegant and ingenious solution to a difficult and quite novel problem are given by its designer, Len Hibbard, in a paper which contains full references to earlier contributors [43]. A photograph of the disc from this paper is shown in Fig. 14, together with a diagram from Ref. [40].



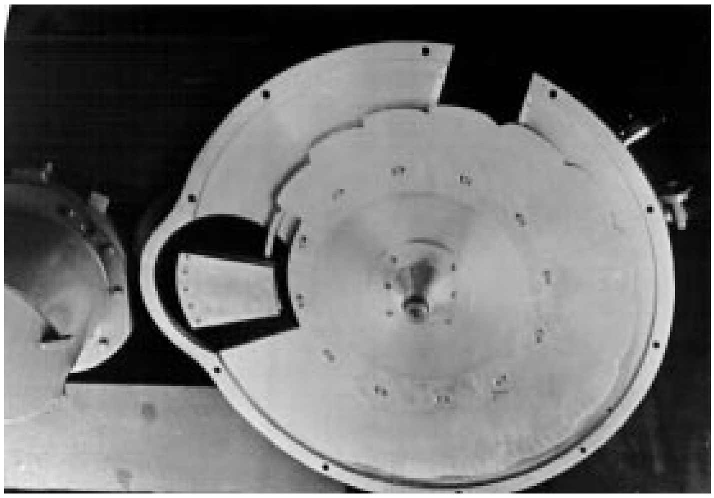


Fig. 14: Variable capacitor for generating the accelerating radio frequency. The capacitor is attached to the open end of a high-Q coaxial line. The stepped rotor is coupled capacitively to the coaxial outer. (From Ref. [43].)

In the three years between the departure of Oliphant and the first operation of the machine the team worked hard, facing, and overcoming, a number of unexpected problems. Some aspects went smoothly, the vacuum design by Len Riddiford [44] and the 500 kV injector by Colin Ramm [45] proceeded as planned. (The 500 kV set had earlier been used for nuclear physics experiments.) In other areas there were problems; the most notorious of these was the ‘pole-face’ disaster. When the magnet was activated the pole-faces, specially shaped and made of $\frac{1}{8}$"-thick soft-iron plates, broke away from their relatively light securing brackets and crashed against one another. The reason for this surprising effect was discovered after ‘a few hours hard thought’. In a more accurately constructed machine the pole tips would all hold firmly to the yoke and no clamps would be needed. The yoke plates were not of the same length, however, giving rise to an irregular gap between yoke and poles. Flux concentrated where the gap was small, leaving a weaker field in the large ‘accidental’ gaps than in the main gap, and this forced the pole plates away from the yoke. The cure was simple in principle, the insertion of a few millimetres of plastic between poles and yoke to reduce the degree of irregularity. Its execution, however, turned out to be very time consuming and resulted in a delay of many months to the project. Details may be found in Ref. [39]. One consequence of this delay was that with the magnet unavailable, it was not possible to test the motor-generator set to peak current. When ultimately this was tested bearing problems were found in the generator which had to be remedied by the manufacturer (Parsons), causing further delay. After these problems were finally remedied and the ‘log jam’ had been cleared, Moon enlisted the work of the whole department, and progress was rapid.

At last, in July 1953, an internal beam of about 10^9 particles/pulse was accelerated to full energy just short of 1 GeV. This was a notable achievement after seven years of hard work by an indefatigable team, though one whose members were often changing. Indeed, few members were there during the whole period. A photograph of the completed machine is shown in Fig. 15, and selection from a list of parameters issued when the machine started is reproduced in the table. A short description of the

machine at the time of its start-up was published in ‘Nature’ [46], and further details and background information can be found in Ref. [47]. Detailed technical references are also given in Ref. [1].

DATA ON BIRMINGHAM PROTON SYNCHROTRON

General Particle Properties	
Estimated maximum energy	1000 MeV
Period of acceleration	1 sec
Repetition rate	6 per min
Energy per rev. (mean)	200 eV
Number of particles accelerated	3×10^9
Magnet	
Maximum usable radial space in magnet gap	33 cm
Value of n at mean orbit	0.68
Total weight	810 tons
Maximum field strength	12 500 gauss
Magnet gap	21 cm
Radius of magnet	16 feet
Peak current	12 500 amps
Peak voltage	1100 volts
RF System	
Initial frequency	330 kHz
Final frequency	9.3 MHz
Voltage on Cee	240 R.M.S
Angular length of Cee	96°
Peak anode dissipation of amplifier	10 kW
Injection – Cockcroft-Walton Set	
Injection energy	460 keV
Magnetic field at injection	217.5 gauss
Vacuum System	
Number of 15" oil diffusion pumps	5
Average pressure in donut	8×10^{-7} mm
Total volume of donut and manifolds	4000 litres
Pumping speed at manifolds	10 000 1/s
Cost	**About £250,000**
Date of first operation at full energy	**16 July 1953**

Of course there was disappointment too that for more than a year already the Brookhaven Cosmotron had been operating at twice the energy and much higher intensity. Furthermore, it was now realized that space would not permit an electrostatic extractor as originally anticipated, and only a relatively feeble scattered external beam appeared to be possible. Both injection and extraction on the Cosmotron had been aided by the incorporation of four straight sections, a possibility not appreciated at

the start of the Birmingham machine. A further feature which caused much embarassment was the very large fringing field which extended a long way outside the magnet. This again was not anticipated at the time of the magnet design. It could have been greatly reduced by providing reversed current windings on the outside of the magnet, as was done on the Cosmotron, and is indeed now general practice.

During the 14 years of operation of the machine a number of improvements were made which greatly improved its reliability, and increased the current available for experiments. A completely new ion source was built and a much more efficient extractor provided, in which a coil was plunged into the magnet after the beam size had contracted, and then energized to reduce the guide field locally and thus eject the particles. A 'flat top' to the magnetic field-time profile was added to lengthen the extracted pulse. The rotating condenser in the RF system was replaced by a flexible function generator that enabled deuterons also to be accelerated, and the coil that dipped into mercury was replaced by a system using ferrites. A detailed description of these later developments is outside the scope of this history, but information on them may be found in Ref. [37] and departmental theses. A paper written in 1955 for experimentalists to present the capabilities of the machine contains a list of acknowledgements to those who contributed to its design and construction, including a number who are not specifically acknowledged in this report or by earlier references [48].

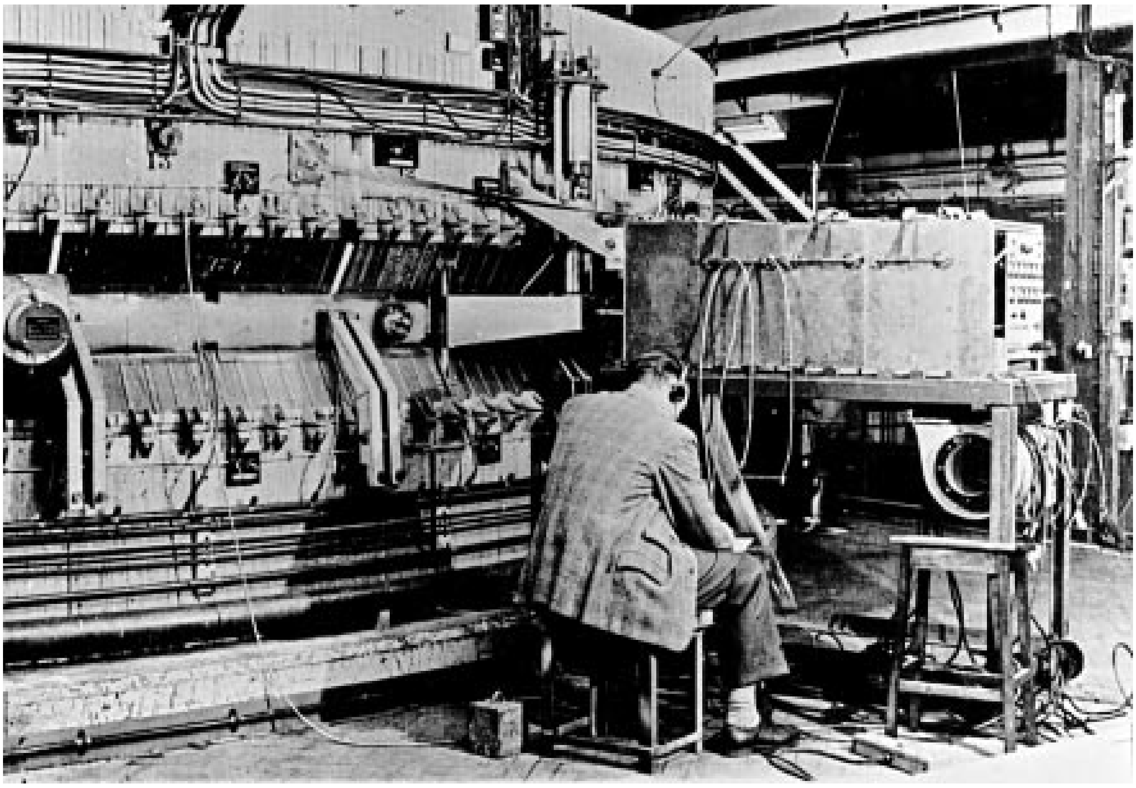


Fig. 15: The completed Birmingham synchrotron. The case containing the variable inductance for tuning the RF can be seen bolted to the top of the magnet.

Reference [37] includes some information on the physics programme on the synchrotron which was under the supervision at first of P B Moon, later of G W Hutchinson and finally of W E Burcham. It was determined by the maximum (external) proton energy of 970 MeV, which was below the strange particle threshold, and by the beam intensity, which was not high enough to provide useful secondary pion fluxes. Under these circumstances the main field of work had to be the nucleon–nucleon interaction. The earliest experiments with scattered-out protons and emulsion detectors were poor statistically but yielded total (and some differential) cross-sections for the elastic and inelastic proton–

proton interactions and succeeded in demonstrating spin-polarization of the scattered beam. The proton–neutron interaction was investigated using a deuterium target. Improved statistics came from the use of diffusion, bubble and spark chambers, from the development and use of fast counting systems but above all from use of the plunging coil extractor. Both double scattering and triple scattering studies were made. Results were analysed by optical model techniques and information was obtained on such topics as charge independence of forces, Coulomb-nuclear interference in scattering, validity of dispersion theory predictions, the $\Delta(1232)$ nucleon resonance and the possibility of a pion–pion resonance. Deuterons of energy 650 MeV were used to test stripping theories and to investigate isospin selection rules.

From 1963 onwards the Birmingham Synchrotron programme began to transfer to ‘Nimrod’ at the Rutherford Laboratory and then to CERN. In the preceding decade the machine had made possible a useful though not spectacular contribution in a specific field, and its existence had led to the emergence of a strong and experienced research group with potential for future work.

The project has, of course, been criticized on the grounds that the style of working was inappropriate to an installation of this size. The more conventional and thorough approach to the Cosmotron, with organized engineering support, is more likely to be successful in reaching its targets in time. Though this is no doubt true, it would hardly have been possible for Oliphant to set up such a costly organization in post-war Britain in a University setting. The enterprise can be seen then as a bold and courageous attempt to be first with a 1-GeV machine. Though at times irritatingly stubborn Oliphant was an inspiring leader, with great faith both in ‘fire in the belly’ as a receipt for getting things done quickly, and in the rapid emergence of good ideas to circumvent difficulties as they are encountered. He was fortunate to have colleagues able to select from his flood of ideas those which were worth pursuing, and strong-minded enough firmly to reject the others. Hibbard was outstanding in this respect, and contributed to all aspects of the design. After Oliphant’s departure Moon, though not participating in the detailed design and not enthusiastic about Oliphant’s style, provided continuous encouragement and gave high priority to providing resources and support.

8 WORK AT HARWELL FOR CERN, 1951–3

The events leading up to the formation of CERN in 1954 are set down in detail in the official ‘History of CERN’ [49]. Before this date there was not only extensive international discussion and diplomacy by senior European scientists, with advice from the USA, but also considerable technical activity towards the design of both the 600 MeV synchrocyclotron, and the proton synchrotron now known as the PS. Here the emphasis is on the technical issues involved, and the organizational background will be only briefly sketched. (A fuller summary, based on the official history, may be found in the biography of John Adams [50].) The account here extends to October 1953, when the hitherto dispersed members of the ‘provisional’ CERN team left for Geneva.

By early 1951 the idea of a European Laboratory to build a high-energy machine was well established, and responsibility for co-ordinating plans was with Pierre Auger, who was Director of UNESCO’s Department of Exact and Natural Sciences. Several potential members of the team to study the machine had already been identified. In May 1951 a meeting was organized in Paris by Auger to discuss the proposed European Laboratory; representatives from a number of countries were present, and it was decided to plan for a high-energy synchrotron, with an energy between 3 and 6 GeV. This was followed by a larger meeting held in October also in Paris, at which it was proposed that the new laboratory should contain both a synchrotron of energy 5 GeV and a synchrocyclotron of 500 MeV.

Estimates, more detailed than those of the May meeting, were made of costs and staff requirements. The names of people who might be asked to participate in the study groups were put forward. Discussions continued, and by May 1952 the first meeting of the Council of the group shortly to be known as ‘Provisional CERN’ was held in Paris. Four study groups were set up. The Norwegian Odd Dahl was nominated ‘Head of the study group in charge of studies and investigations regarding accelerators of particles for energies greater than 1 GeV’. His deputy was Goward, with other group members Hannes Alfvén (Sweden), Wolfgang Gentner (Germany), Edouard Regenstreif (France) and Rolf Wideröe (Norway) [51]. Their remit was to study the design of a machine similar to the Cosmotron, but with higher energy.

At the second Council meeting, which took place some six weeks later in Copenhagen, the machine energy was fixed at 10 GeV, and further members with specific expertise in accelerators were added to the team. These included D W Fry from Britain, who was head of the General Physics division at Harwell which included the Accelerator groups. Also chosen were Kjell Johnsen an accelerator theorist from Norway who had already assisted Dahl in his planning for the proposed laboratory, and Chris Schmelzer a German with experience of radio-frequency applications. Fry responded by asking John Adams, who had made a major contribution to the design and operation of the 175 MeV Harwell synchrocyclotron, to look at the magnet design for the proposed European synchrotron. At this time Adams was engaged on designing a high power klystron, based on the design at Stanford, for a proposed high-energy electron linear accelerator; the accelerator itself was the responsibility of Goward. (This accelerator and the klystron project were later abandoned, after the realization that the use of quadrupole focusing would make a proton linear accelerator feasible, and in the belief that this would be a more interesting option.)

A very important development occurred in the middle of 1952; Dahl, accompanied by Goward and Wideröe, made a visit to Brookhaven in August to see the Cosmotron. When they arrived they learned of a new concept just discovered at Brookhaven to be known as ‘strong focusing’ or the ‘alternating-gradient’ principle. By greatly strengthening the gradient of the magnetic guide field and also alternating it around the circumference a much greater net focusing force in both horizontal and vertical planes is generated, so that a much smaller space for the orbits, and hence a smaller magnet, is required. The improvement was dramatic; the basic orbit dynamics and speculative parameters for a machine of energy 30 GeV with internal aperture of the vacuum chamber only 2.5×5 cm had been worked out and presented in a paper submitted to the ‘Physical Review’ on 21 August by Courant, Livingston and Snyder [52]. There were two features of the new machine that later gave grounds for concern. First, the very strong focusing implied that the number of betatron oscillations per circuit of the machine greatly exceeded unity, and decreased as the magnet saturated and the field gradient decreased. Second, because of the very small amplitude of the betatron oscillations the phase-focusing corresponded to that in a linear accelerator, where the stable phase occurred when the accelerating field in the accelerating cavity was decreasing in time. At extreme relativistic energies, higher than that of the proposed machine with the original parameters, there would be a ‘transition energy’ at which normal synchrotron phase-focusing on a rising field would occur.

Dahl returned to Europe full of enthusiasm for the new concept and eager to explore its feasibility for the new machine. By October he was ready to put his proposals to the Council, who sanctioned his proposal for a 30 GeV machine and entrusted the design to his team. This immediately changed the balance of the work that was required to be done, implying a much larger component of ‘machine physics’ as compared with engineering design. What was needed was far more than the simple scaling up of a machine already working, and built on well understood principles. European accelerator physicists were keen to study and explore the new idea.

At Harwell Goward quickly aroused interest in the new principle, and this was enhanced by a visit by Courant early in November. Meanwhile the first indication of future complications had occurred. Lawson, though no longer working on accelerators, had earlier studied forced oscillations on the Malvern machine and he quickly realized that as the number of betatron oscillations per revolution passed through an integral value small errors in the magnet alignment or field value would cause resonant build up of the oscillation and the beam would strike the vacuum chamber. After discussion with various colleagues a brief note was written [53]. One suggestion that had been made in discussion was that the focusing field should be non-linear, so that the effect of a resonance would be limited. In his note Lawson assumed that this would give a random build up of amplitude, and that even this would be unacceptable. This hypothesis was not generally accepted; indeed, what would happen was not clear, and this gave rise to some intensive study of non-linear oscillations. Many proposals were explored for overcoming or mitigating the difficulty.

Now that the design of the machine was seen to involve new and unknown features the study group was extended, and contained a number of part-time participants. It was clearly necessary to proceed to quantitative studies so that a set of parameters could be chosen, and to assess the full significance of the resonances. Adams, who was concerned with the magnet was clearly deeply involved, and he was joined early in 1953 by Mervyn Hine another ex-radar scientist who had been working on the abandoned 600-MHz electron accelerator at Cambridge. Niels Bohr, head of the CERN theory study group arranged for Gerhard Lüders from Göttingen and T Sigurgeirsson from Iceland, to work in Copenhagen on orbit dynamics. At Harwell John Bell also contributed to the orbit theory, and in January 1953 wrote a report on the algebra of strong focusing, which contained a derivation of what is now known as the Courant–Snyder invariant [54].

During 1953 the design team was in several locations. Dahl remained in Norway at the Chr. Michelsen Institute; he had reacted enthusiastically to the idea of building a strong-focusing machine, and was keen to pursue the engineering design. Johnsen remained there also. The theoretical group was based in Copenhagen at Niels Bohr's Institute, and the British team remained at Harwell. Regenstreif continued to work in Paris in Pierre Grivet's laboratory at the Sorbonne studying orbits, magnets and profiles. Work on radio-frequency problems was centred at the University of Heidelberg under Ch. Schmelzer. Close touch was maintained with Brookhaven, and it was agreed in March that John and Hildred Blewett, both major contributors to the Cosmotron, would help directly in the European project, and would come first to Bergen in April and then move to Geneva later in the year when the other teams assembled there.

The year 1953 was a busy and stimulating one. There were two achievements of the British study group. First, new features of the orbit dynamics were discovered and investigated, and second, the theory was used to calculate actual parameters for a realistic design of a machine for 30 GeV, including tolerances and engineering constraints. During the year a number of meetings were arranged and numerous informal reports were written. It is not clear how complete a record these provide. On the theoretical side Lüders and Sigurgeirsson (who introduced the concept of 'admittance') [55] together produced a formal theory of orbits in periodic structures, incorporating effects of misalignments responsible for the integral resonances, and also errors in gradient which also gave rise to half-integral resonances [56, 57]. These were at the same time identified by Hine using more intuitive arguments; he also raised the question of higher-order subharmonic resonances. Hine working closely with Adams embarked on a study of non-linear effects, making for the first time the extensive use of computation on ACE, the 'Automatic Computing Engine' at the National Physical Laboratory. This work is preserved among a series of papers, all jointly by Adams and Hine, in which a large number of effects, such as vertical–horizontal coupling were investigated [58, 59]. These studies were accompanied by parameter surveys and analysis of tolerances appropriate to an actual machine. Over the year the value of n

assumed neglecting the resonances was considerably reduced, leading to an aperture of 7×15 cm rather than 2.5×5 cm in the original proposal. Nevertheless, this represented a very substantial improvement compared with what would be required in a weak focusing machine. By the time of the move to Geneva in October, the parameters of the PS had essentially been fixed.

Elsewhere other factors were being considered, such as the design of the radio-frequency system. One new problem that arose was that the 'transition energy', where the stable phase changes over from being on a falling field in the resonator to a rising one occurred at 6.7 GeV, now less than the machine energy. The question arose as to whether this could be crossed without loss of the beam, and detailed analysis was required to produce a reassuring result. This problem was addressed particularly by Kjell Johnson in Bergen, who also investigated other aspects of the dynamics in the radio-frequency field, such as the behaviour at injection.

Goward, as well as his general duties as Dahl's deputy, studied the possibilities of aligning the magnets using the beam as a monitor. Engineering topics such as details of magnet design and power supply requirements were studied by Dahl, and trial machine layouts sketched. Regenstreif continued with non-linear orbit dynamics and model magnet studies.

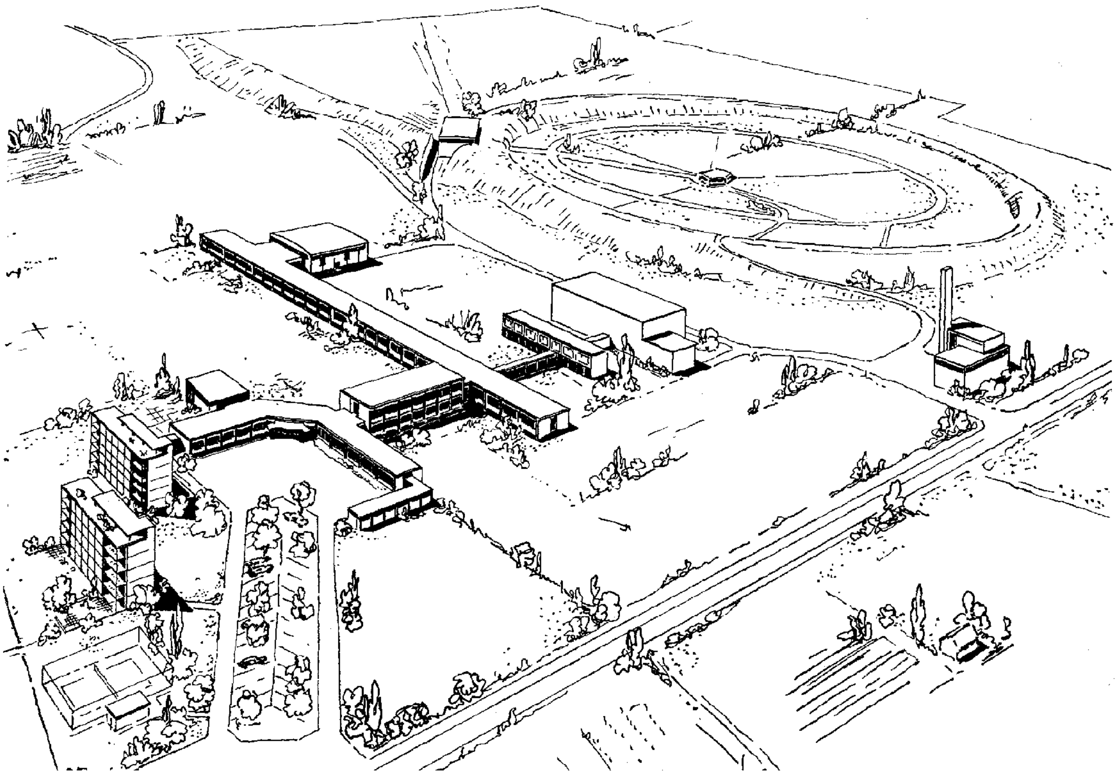
During 1953 meetings between the sub-groups had been held, at least two of these being in the UK. Records survive, and the agenda and minutes give a good impression of what the various participants were doing [60]. One such meeting was held at Harwell by the orbits sub-group on 1 March. In addition to Harwell staff, Johnsen and Regenstreif were present, and three members of the theoretical group, Jacobsen, Lüders and Sigurgeirsson. Several conclusions are reached: first, the prospects for making a strong-focusing synchrotron are good; second, because of alignment difficulties, n should be reduced by 4 to 900; third the magnetic field could be non-linear, but if so it must be closely controlled; fourth the frequency and phase need to be carefully controlled in passing through transition energy and finally, the field inhomogeneities at injection will require an injection energy of 50 MeV rather than 4 MeV as previously assumed.

Just six months later, in September, there was a further discussion but with no member of the theory group present. It was attended by the Blewetts, who had been working with Dahl and Johnsen in Bergen since July. The neatly handwritten summary by Adams begins: 'It is becoming possible to choose some of the critical parameters of the CERN proton synchrotron by scientific arguments. In view of the coming presentation of our progress to the CERN Council the above group members met to agree on a set of parameters that could be used to illustrate the theoretical work completed to date' [61]. A summary of proposed parameters, essentially those of the final machine, is appended. The meetings mentioned here were held at Harwell; others were held elsewhere, dealing with other aspects of the machine, for example its layout and shielding requirements, and the design of the radio-frequency system. Some details may be found in the CERN archive.

It is difficult at this time to chronicle the details of this very eventful year, and to apportion credit in an authoritative way. One factor to be remembered is that the alternating-gradient idea came from America, and the staff of Brookhaven and elsewhere were generous with their information and help. Nevertheless, it is generally accepted that the British contribution of Adams and Hine, who worked together as a very powerful combination, was an important one in defining a set of realistic parameters for the machine. They insisted on deep understanding and cautious realism in practical matters; this extreme caution did not always endear itself to the Americans, who had been encouraged by the successful operation of the Cosmotron, which had also faced many unknown factors at its inception. This gave rise to Hildred Blewett's famous remark about the 'miserable English' [62]. Adams himself

confesses to 'Jeremiah-like prognostications' concerning inhomogeneities, together with Hine and Lawson [63]. (Lawson, whose single contribution had been a negative one, was no doubt influenced by his earlier disastrous entry to the field of accelerators, described in Section 3.)

In October 1953 the team that was to design the machine assembled in Geneva. This did not include all who had been working in the study group, notably Odd Dahl, who resigned his appointment shortly after, nor the theoreticians who had been working in Copenhagen. It did include, however, a number of others who had so far not been deeply involved. In a list provided at the time, 17 technical staff are listed, together with seven consultants. Their accomplishments, however, are well exhibited in the series of lectures presented at the Conference held in Geneva at the end of October [64]. Included is a historical review of the project by Dahl. Many of the speakers had no previous experience in accelerator design, furthermore the team consisted of a number of sub-groups in different locations; communication was not so easy as it is today. Despite some tensions, noted in the Official History, the team had worked well together, and laid the foundations for a remarkably successful outcome. John Adams was to play the central role, not only in guiding the technical design of the machine and its buildings, but in integrating an international team at a time when memories of recent hostilities were not far distant, and collaborations on this scale in scientific enterprises was something quite new. In conclusion, Fig. 16 shows how the laboratory was envisaged at the time [65].



LE LABORATOIRE DE MEYRIN
1. A l'extrême-droite : le Synchrotron à protons sous son remblai circulaire. — 2. A droite, en bordure de la route : la centrale électrique. — 3. Au centre : le synchro-cyclotron, les laboratoires et les ateliers. — 4. A gauche : le bâtiment de l'administration et les buildings d'habitations, cantine, etc...

Fig. 16

Acknowledgements

Detailed acknowledgements to those who helped me by providing information used in this report are set out in Ref. [1]. These included many reminiscences of people who worked on the machines described. Formal acknowledgements for copyright material are due to the UK Atomic Energy Authority plc for permission to reproduce Fig. 4 from Ref. [10] and to the Institute of Physics for Figs. 1 and 6 from Ref. [8].

References and Notes

The following abbreviations are used in the references:

AERE, Atomic Energy Research Establishment, Harwell, now AEA Technology, plc.

CERN/ARCH/-, CERN Archive. Papers relevant to this report, many of which are cited in the references below, have been placed in box JDL0001. Copies of some may be elsewhere also. Any new interesting material found by the author after this report is written will also be placed here.

HMSO, Her Majesty's Stationery Office.

PRO, Public Record Office, Kew.

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